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SOME POINTS IN THE ETIOLOGY OF TYPHOID FEVER.

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The object of this paper is to briefly review some of the opinions entertained by the medical profession on the subject named, and, if possible, to contribute a single mite toward the settlement of some of its vexing questions. Scarcely a journal comes upon our table that does not contain some discussion on one or more of the topics involved; and the difficulty of classifying all the arguments and weighing the conflicting testimony becomes, to the busy practitioner, so great that he is tempted to abandon it altogether.

Probably no disease is so unsettled in regard to its causation as typhoid fever. I propose to classify, if possible, these existing opinions, and attempt the task of bringing to view the doctrines of each, weighing and comparing all with a spirit of strict impartiality. Individuals are nothing, facts everything. Nevertheless, medical men will differ, as they have since the world began. One forms a theory, another directly opposes it; a third sweeps both away in a single sentence; and in the meantime typhoid fever moves on in its work of carrying away more than a hundred thousand of the human family every year, thus urging upon us the necessity of making haste to uncover the hidden truths and arrest such a frightful sacrifice of human life. In the present state of our knowledge, can this be done? Viewing its etiology in one light, we may answer, yes; in another, no, only

as we can improve in our knowledge of its nature and treatment.

If we believe, with Budd, Parkes and others, that it has the same contagious nature as smallpox; that with certain precautionary measures and the free use of disinfectants the disease may be wiped out entirely from the face of the earth; certainly it is our duty to understand and follow well these directions. If, on the other hand, we believe, with Greisinger, that typhoid, aside from its contagiousness, may and does often originate spontaneously, certainly we cannot hope for such a desirable end. Again, if we believe, with Edwin Snow, Registrar of Providence, that this disease "arises solely either from cesspools, privy vaults, sewers, or chiefly from the decomposition of vegetable substances," then our only hope rests simply upon our skill in treatment.

By examining carefully the medical literature upon the question of etiology, we find four classes of opinions:—

1. Non-contagionists.
2. Direct contagionists.
3. Miasma contagionists, of Liebermeister.
4. Contagio-pythogenicists.

The Non-contagionists, represented by Ewen, Wood, Lindwurm, and Huss, class typhoid with such diseases as pneumonia, miasmatic fevers, etc.

The direct contagionists, represented by Budd, Roberts, and others, class typhoid fever with smallpox, typhus, scarlet fever, etc. This class is not large at present, and is undoubtedly finding less and less advocates, being properly a relic of the time when the distinction between typhus and typhoid was not generally made; the former

of which is everywhere acknowledged to be contagious.

The miasma contagionists, properly represented by Thiersch, of Erlangen, and Liebermeister, are entirely opposed to the doctrine of direct contagion from person to person, as in smallpox, but believe in a specific poison of such a nature that it is only perfected and rendered operative some time after leaving the body of the typhoid patient.

The contagio-pythogenicists, represented, as I deem it, by Trouseau and Niemeyer, maintain that typhoid, while it is communicable by contagion, may, under favorable circumstances, originate spontaneously.

The great importance of the subject will lead us to examine it somewhat carefully; divesting ourselves of prejudice, as Liebermeister so frequently urges in bringing forward his carefully collected and powerful arguments, so that facts and not theories alone shall aid in building up a belief which shall direct us in a practice tending to eradicate from the human family what is eradicable.

Contrary to the opinion of the first class, the non-contagionists, we remark, that typhoid fever, as well as cholera, dysentery, and yellow fever, must possess some sort of contagion, either direct or indirect; some specific power of propagating itself by generating its own specific poison. This we think is, at the present day, almost universally admitted; a general belief, pervading the laity as well as the profession in all countries, founded upon the accumulated experience and observations of the last quarter of a century; and hence, it finds its expression in the rules of all quarantine and health boards, so far as we know. Therefore, we need not spend much time in discussing the subject, any more than to name the following points in proof.

1. Typhoid fever, as in other contagious diseases, very seldom recurs in the same individual. In thirty-five years' experience I have never but once seen a person attacked a second time. (Budd, Liebermeister, Wood, Flint, Alonzo Clark, Aitken.)

2. As in other contagious diseases, this is attended with a constant eruption on the mucous membrane; generally, also, on the skin.

3. Typhoid may very frequently be traced to a preceding case, as in other contagia, and could undoubtedly be commonly so traced but for the special nature of the contagious principle, it being only *partially* developed on leaving the infected body; and

4. A focus of infection, either from the excreta

of the diseased body or of spontaneous origin, once made known and removed, the further spread of the disease is arrested. Such facts are not rare, and will be again referred to in this paper.

Next, in opposition to the doctrine of the direct contagionists, it seems evident, at least to my own mind, that the very broad ground taken exposes them to attacks from so many quarters that their system is rendered quite untenable. In classing typhoid fever with smallpox and syphilis, the very types of all contagious diseases, they must show that in the large majority of exposures the disease must be contracted. This must be the rule; the exceptions very few indeed. Now, in order to prove this they must show, first, that a case occurring in a family must produce the same in all the other members who have never passed through it; and secondly, that for every existing case they must find one that preceded it, and between these two an actual contact. Such strong doctrine is, however, modified by some, who believe, like Flint, that typhoid is only *mildly* contagious, many contracting it after fair exposure and many escaping. This line of theory is harder to deal with, for it is easy to admit of many exceptions. Even these however, must show that it is exceedingly common to see attendants, visitors and physicians falling under the disease. I am not aware that they have been able to do this. Contrary facts however, are abundant, and will be presently referred to. I have, myself, witnessed, in private practice, something over six hundred cases, from almost as many different families, and having never seen, except in comparatively a very few instances, the occurrence of more than one case in a family, and these exceptions mostly traceable to a focus of infection, I cannot but form the belief, indeed the firm conviction, aside from the mass of testimony from others, that there is no such thing as direct contagion.

Visitors and physicians enjoy almost a perfect immunity from the disease; even those who wash the soiled clothes for typhoid persons are remarkably exempt, unless they have remained unwashed for too long a time. I have always permitted friends to call, with the assurance that the danger was simply *nothing*. Liebermeister never saw "a single hospital patient, physician or nurse attacked with typhoid fever, although such cases were placed in the general wards." Murchison's experience was the same, with the exception of eight cases only, among two thousand five hundred and six. This small

number will certainly allow us to assume that these originated probably from some local cause or condition, and argues strongly against direct contagion. Andral and Chomel are equally emphatic in asserting like experiences. Therefore, it seems far more scientific to look for some other explanation than one that has such a preponderance of exceptions. True science, we think, never admits of such loose reasoning as this.

The doctrine of the third class, namely, that of an intermediate development of the specific contagion, that is, the necessity, in order for its final perfection, of a period of existence outside the body, is really ingenious, and has many points that are strong, if not incontrovertible. It is a doctrine fully discussed in Ziemssen's *Cyclopedia*, by that most careful observer, Liebermeister, and although not new, is no doubt eliciting the careful attention of the medical world. It very readily explains why typhoid, cholera, etc., are infectious, though not directly contagious; why the contagious particles are capable of maintaining a separate existence after leaving the body for an indefinite time, and even of mingling with streams, reservoirs or underground currents of water, and infecting persons at a distance and entirely separate from any typhoid patient; how an epidemic or endemic may be originated in some isolated place by means of fomites, or by the purchase and use of contaminated milk, or by the fecal deposit of some typhoid patient some time previous, or by the use of water from some infected well or fountain. Plenty of proof from actual occurrences can be cited upon all these points. In most of the later writings on the etiology of diseases of the class named may be found descriptions of such instances. Similar instances are no doubt familiar to all who have sought for them during an epidemic particularly.

If what has been said under the head of direct contagion be true, namely, that it is an impossibility, it is very evident that the virus must be imperfect and inoperative at the time of its leaving the body. It is equally evident that it must afterward attain its perfection, otherwise there would be no contagion at all. The adherents of this theory of an intermediate state of the specific virus bring as proof not only what has already been said in relation to this point, but that comparison is readily drawn from the animal kingdom; facts that are well known. The tape worm, insects, etc., for instance, have different stages of existence; the tape-worm, after leaving the ovum, penetrates the fleshy tissues and be-

comes encysted; so remaining until the flesh containing these cysts is eaten by some other animal, when the perfect worm is developed. Insects have the larva, or worm stage; the pupa, or transforming stage; then the perfect insect. So with the germ, if it be a germ, which we doubt, of typhoid virus, as will be seen further on. It comes from the body in the first or imperfect stage, finds a favorable soil in privy-vaults, sewers, or other places where there is decaying animal or vegetable matter, and there grows into its perfect or active state. It may then find its way into streams or reservoirs of water, or be carried by the air ready to infect the human body. Numbers of instances might be cited showing clearly the formation of such centres of infection. I have myself known instances of this kind. One I will mention, that occurred in the country. A case of typhoid appeared in a family of eight persons. In about two weeks another occurred in the same family; a careful examination of the surroundings impressed the idea that the cause existed in a well of water used by the family. Soon another came down with the disease, and still others were complaining of slight indisposition. I ordered them to discontinue the use of the water from this well, and no more cases occurred. The well was suspected on account of its near proximity to a privy vault. Dr. Stoddard, in the "Transactions" of the New York State Medical Society, records a similar instance in Rochester. Others are familiar, where milk diluted with infected water, or the milk-cans washed with such water, have caused cases from which many others were infected. Instances are too numerous where schools, families and towns have been infected from like causes.

But while this very plausible theory is capable of explaining nearly all the points involved in the discussion; while its ingenuity and application are beautifully adapted to arouse the attention of physicians to the preventability of typhoid, and will, therefore, surely result in good, by greatly lessening the number of cases, yet, it seems probable, at least to my own mind, that it cannot meet *all* the requirements of a settled and universally adopted theory. Every philanthropist could well wish it true, because, involving as it does, a continuous chain of actual cases, it is plain that if this chain be broken fairly; if in the few last cases every specific germ or particle be destroyed, the fond hopes of its advocates would soon be realized, and typhoid fever would be among the things of the past. We fear, however, that the thought is too flattering to be admitted among the grand possibilities; that it

makes the herculean task too easy. And while we might pray for its speedy realization, causes for doubt will force themselves upon us almost every day.

Hence we come to notice, and to adopt, if you please, the theory of the fourth class above mentioned, viz.: the contagio-pythogenicists, who adopt the doctrine of miasma-contagion, as just described, *with* the doctrine appended to it that, under favorable though perhaps rare circumstances, the typhoid virus may be formed and perfected away from and entirely independent of any person having the disease; in other words, that it may originate spontaneously, and when thus originated, it is capable of generating itself in the body of an affected person, just in the same way as others who contracted the disease from actual contagion.

Without admitting the possibility of spontaneous origin, how can we explain fairly and without any labored hypotheses, those isolated cases that so commonly occur in the open country, here and there one, widely separated, far away from every exposure, among the steady farmers, who are always at home; or among lumbermen, perhaps, far away in the forest, dwelling often miles apart? Such facts are exceedingly common, and they are repeated year after year, leading the mind, I was about to say, inevitably to the conclusion that they originate *de novo*. Why must we strain every power of conception to believe that a preceding case brought such an one into existence? Surrounded by such striking and often-repeated facts, it would seem *natural* to deduce the doctrine of spontaneity.

I use the term "natural," and will illustrate by supposing a case. Let ten physicians, without special instructions in the etiology of typhoid, commence practice, specially in the country, "which is the proper place," as Dr. Dorset, of England, remarks, "to form correct conclusions in the matter," and nine of the ten will form the opinion that it arises spontaneously. In densely populated cities it is no wonder that opposers of the doctrine of spontaneity are numerous, and find little difficulty in believing that all the cases that occur are really connected one with another, because contact, directly or indirectly, is so easy, and that it is not necessary to even dream of the possibility of spontaneity. But in the country, open and sparsely settled, it is far otherwise, any contact between them seeming to be almost an impossibility; and while the plain fact may be and is urged with all the force of words, little heed is given to it by those whose professional lives are spent

among the crowded thousands; such continue their skepticism because they view the subject from one side only, and are unable to look at it from the other.

Much additional testimony on this point might be brought forward from others; those, even, who differ widely on other points. Murchison, of the London Fever Hospital, whose opportunities for studying this fever were equal, at least, to those of any other man, has staked his reputation by giving it the name of "pythogenic fever," meaning, of course, a fever arising from putrefaction. Snow, of Providence, City Registrar, who is a careful observer, and has good opportunity for studying this disease, publishes the same opinion, in very decided language. Dr. Barker, of Bedford, England, believes also in spontaneous origin. Dr. Piedvacke, another most careful observer, found at least twelve cases out of three hundred and twelve, that by the most careful search could *not* be traced to any anterior connection, direct or indirect, with any other patients." Even one such case, really genuine, would, of course, settle the question. Sir Wm. Gull calls it "the filth fever." Flint maintains that "it frequently or generally originates spontaneously." George B. Wood speaks even more decidedly. Tanner says it is generated by putrefying animal matter; and it is well known that Trousseau, Bennett and Niemeyer maintain the same opinion.

An argument made use of by the opponents of spontaneity is, that the spontaneous generation of typhoid can no more occur than the spontaneous generation of plants and animals, the possibility of which is now abandoned as a myth; that though the disease was first originated or created, so to speak, it cannot be *recreated*, any more than the Almighty will recreate an animal or plant that already exists. The force of this argument we must admit, were it not that it has the fault of being predicated upon an assumption, that the specific virus is living or organized matter. This, as all know, is a revival of the ancient doctrine of a "contagium vivum;" certainly we have no objection to this if the doctrine be the true one. But we have no more right, and certainly no more reason, to assume that the contagious matter is organized, than we have to assume that it is a chemical or matter that is unorganized.

What the chemical is, if it be one, may never be determined, for certainly it has not been as yet; some have supposed that certain gases might be acting agents; and various ones have been tried on dogs, birds and other animals, by

Drs. Barker, Murchison and others, but all such experiments failed to produce symptoms like typhoid fever. The probability is, that it is by no means a common product; but on the contrary very rare, seldom, and only under the most favorable circumstances, formed.

Now it cannot be disputed that a specific inorganic body can produce a specific effect upon the human organism as well as a germ. That it can, we see every day, in our practice. Both organic and inorganic remedies affect particular organs, no matter how applied or introduced in the body. Thus, we have cathartics, diaphoretics, diuretics, etc. Probably every article has its special action upon a special organ; hence we are able to modify the action of the heart, the kidneys, the skin or the bladder. Why may not a certain chemical agent act through the body upon the glands of Peyer, producing typhoid; the poison discharging itself there in a condition ready to go through some specific change and become a real contagium? The same reasoning that applies in one case applies also in the other, and the theory of the spontaneous origin of the contagious virus, which has been held from time immemorial, be sustained.

A query may arise in reflecting upon the details of this supposed chemical; how can it (the virus) enter the body perfect, pass out imperfect, destitute of the contagious principle, and then become perfect again outside the body? The answer is, that very many substances are changed in their course through the body, as is well known; some in the stomach, some in the intestines, some in the blood, and some in the organs or tissues by which they leave the body. Benzoic acid, for instance, is eliminated by the kidneys as hippuric acid. Indeed, many are the changes that must occur. In order to make more clear the idea, we will imagine a case such as may, and probably does happen; similar ones certainly do. Suppose an oxide or a sulphide is taken in the stomach, and in its course through the system the oxygen or the sulphur is separated, the base appearing again in the fæces, having passed through Peyer's glands, or reached the alimentary canal in some other way; or appearing in the urine through the kidneys. This base having an affinity for the oxygen or the sulphur, recombines with it, outside the body. It would again become what it was on entering the stomach. This, of course, is merely a supposition, and without any pretence as to what the contagious substance really is. At any rate, such a conception has an abundance of parallels.

The second objectionable point against the contagium vivum theory which we will mention is this: It requires always the planting of the actual seeds or germs to produce the real contagion; whereas the chemical theory waits for no such thing, as none is required; chemical changes are continually going on; combinations formed and decomposed; changes that probably have never been imitated by the chemist, and perhaps altogether unknown to him.

Such are some of the reasons for adhering to the doctrine of the spontaneous generation of typhoid fever. Others might be brought forward, as for instance: In order to explain the contagium vivum theory, its adherents must assume that the contagious germ is capable of maintaining an existence under a variety of conditions; not only on the surface of vaults, sewers, or cess-pools, but percolating through the dense soil, and far underground; or mixed with various fluids, as milk, water, fecal infusions, etc. Now we know that many of these low organisms cannot maintain vitality under such widely different circumstances. But suppose they allege that the contagium dies, and its substance becomes infused in such media, and is thus transported. We know that organic matter, even in such infusions, readily decomposes when submitted to such varying influences. In either case the assumption is unnaturally or unduly strained to accommodate all the requirements of such an hypothesis.

We repeat, then, the query, Is the contagium of typhoid fever organized matter? We answer, it has not been proved. Neither is there an argument to support it that will not apply to the theory that it is unorganized. But let us invert the proposition. Has the theory that it is unorganized any argument that will not apply that it is a germ? We answer, yes, namely: that thousands of cases occur that, with our best reasoning and closest scrutiny, we have a right to assume originated *de novo*.

We think, therefore, in conclusion, that we have the best and the largest share of evidence, that two of the doctrines that have prevailed in reference to the etiology of typhoid fever, namely, non-contagion and direct contagion, must be abandoned; that indirect or miasma contagion is correct as far as it goes, but that it does not comprehend or contain the whole truth until we add to it the possibility that cases may originate spontaneously. With a theory so constructed, I think we are able to explain, with reasonable satisfaction most, and may we not say all, the intricacies that may arise.

TRANSFUSION—THE INDICATIONS FOR ITS USE AND THE METHOD OF PROCEDURE.

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This operation has been vaguely known to the profession for the last four hundred years, and there are obscure allusions in the Roman poets which would seem to indicate that it was practiced as early as the "Augustan Age."

"Ut repleam vacuas juvenile sanguine venas."
(OVID).

The earliest authentic case on record is, so far as we know, that of Pope Innocent VIII, who was unsuccessfully operated on in April, 1492. The vital powers of Innocent VIII rapidly gave way; he had for some time fallen into a kind of somnolency, which was so profound that the whole court believed him dead. All means to awaken the exhausted vitality had been resorted to in vain, when a Jew doctor proposed to do so by the transfusion, with a new instrument, of the blood of a young subject. An experiment which had hitherto only been made on animals. Accordingly, the blood of the decrepid old pontiff was passed into the veins of the youth, whose blood was also transferred into those of the old man. How it was done the report does not state; but the experiment was tried three times, and at the cost of the lives of three boys, probably, from air getting into the veins, and all without any effect to save that of the Pope.

A Tuscan, by the name of Francesco Folli, in the year 1852, first made a decisive advance, by which theory and vague ideas were abandoned, and gave place to the experiments which Folli in that year practiced in the Court of Florence, in the presence of the Grand Duke Ferdinand, II. In the "Stadera Medica," a valuable work published in 1660, by Folli, he recalls the fact that he suggested transfusion in certain of his works of a date anterior to 1657. That is when the English Wren instituted his experiments, to whom Sprengel, ignorant of this fact, attributes the first experiment. Harvey, about the year 1657, suggested it, and some years later it was done. Shortly after (Lauer), of France, practiced by injecting into one animal the blood taken from another. Folli and Wren limited themselves to experiments on animals and did not attempt to test them on man. The last step was to perform transfusion between animals and man, and finally between man and man. From the success of Douay's (a French surgeon) experiments, great enthusiasm was created, but from the many deaths that occurred, it was, on the 17th of April, 1668,

prohibited, unless by express permission of the Faculty of Medicine of Paris.

The transfusion of blood, from the date of its promulgation in France, Italy, Germany, and England, rapidly roused furious adversaries, who made it a subject of acrimonious disputation similar to what we have seen about animal vaccination. On the other hand, its fanatical supporters, going to the other extreme, beheld in it a panacea which would maintain youth, re-establish health, alleviate mental ailments, change the hereditary disposition in certain affections, and prolong life beyond its natural limits. In 1685 Harwood bled an old horse to the last extreme, then injected into his veins blood from another horse. After the transfusion the horse that had been ex-sanguinated arose and began to feed. As it was in France, so was it in Italy; the transfusion of blood was abandoned by every one, with few exceptions, till the beginning of the present century. For when a man was racked for affirming that the earth moved around the sun, we can readily understand that the members of the healing brotherhood, in the Middle Ages, had to proceed in all their investigations with fear and trembling. In 1802 Michele Rosa renewed the neglected animal experiments, and it is at this time that we see the beginning of the substitution of transfusion from man to man, for that from animal to man. In 1819 Blundell, of London, performed numerous experiments. His cases numbered six; for a time they improved, but eventually they died. These operators all used the method of direct transfusion.

In 1859 Waller injected nine ounces into a patient, which resulted in complete recovery. This was by means of the indirect method.

Martin, of Berlin, has published fifty-seven cases, forty-five of which recovered. The cause of the exsanguination was post-partum hemorrhage.

In 1864 Grailly Hewitt and B. W. Richardson turned their attention to this subject, and in 1868 Braxton Hicks reported six cases; but unfortunately all of his died.

Brown-Séguard has bestowed a great deal of time and energy in experimenting upon this subject, as also the late Dr. Freer, of Rush Medical College, with whom I think it was almost a hobby. But I have not been able thus far to discover any record of his cases, although I think he published a small work embodying his ideas of the subject. Numerous others are working up this subject at the present time, but it is not necessary to give their names.

In spite of its non-appreciation by physicians

in general, there have always been eminent men in the profession who placed in transfusion of blood confident hopes of certain future advantages for suffering humanity. For if facts overcome all arguments, facts authentically established are not wanting, to prove that transfusion performed properly is able, in many cases, to arrest the otherwise inevitable issue.

The most important indication for transfusion of blood is naturally acute anæmia, and the most frequent example of this we find in midwifery. We should be guided here chiefly by the contractions of the uterus. Provided they are energetic, we may delay the operation, but should the uterus be unable to contract, transfusion is the most powerful stimulus for the purpose we have at our command. In great loss of blood after injuries or operations, however, the surgeon should never wait. It appears that the German surgeons employ transfusion much more freely than is generally supposed. For instance, Esmarch, during the removal of a vascular tumor at the base of the cranium, replaced the lost blood by transfusion in the arm, and during an amputation at the hip joint the blood was saved and injected into the body again through the femoral vein.

Leisrink recommends transfusion in convalescence from typhoid fever, or from any long febrile disease which leaves the body so exhausted that any slight intercurrent disease might prove fatal.

Among the qualitative changes of the blood we find transfusion of importance in pyæmia and septicæmia. The transfusion should be frequently repeated. Of course this treatment is useless, provided the source of the blood poisoning remains.

It might also be used in the various forms of gas poisoning, and in such cases as much as possible of the unhealthy blood should be removed at the same time.

CASE 1.—Professor Heuter of Griefswald, related a case of poisoning by CO₂. Patient, aged 26, had been exposed to the gas and found insensible. Attempts to resuscitate by artificial respiration, by compression of thorax, electricity, etc., were made, without success. When Dr. Heuter came to him respiration was very superficial and intermittent, pulse small and frequent, pupils did not act, cornea quite insensible. This was his state half an hour after he was found. The Doctor obtained a pound of blood from a student, defibrinated and injected it at a time when respiratory movements had quite ceased. A vein in the patient's arm was

opened, when about two drachms had been injected, but only a few drops of blood escaped. Artificial respiration was kept up while the injection was being made. After half of the blood had been injected blood flowed more freely from the vein. At the end of the injection the pulse was fuller and slower, and natural respiration returned. In half an hour the pupils were sensitive to light and he moved his arm. Recovery was complete on the fourth day.

CASE 2.—Dr. Buchser, in the *Medical Record*, reports a case of a German girl, apparently moribund from epistaxis, where he injected three ounces of defibrinated blood in the median cephalic vein, with complete recovery.

CASE 3.—Dr. Albert H. Smith, of Philadelphia, reports a case of a patient suffering from an attack of typhoid fever, which ran the ordinary course till the middle of the third week. He then began to bleed freely from the nose and gums, and to pass blood with his urine. All the usual remedies were tried without success, except of a temporary nature. When two and a half ounces of defibrinated blood was transfused, all the oozing from the gums and nose stopped immediately. This is opposed to the opinion of Lesser, in the *Boston Medical Journal*, where he says that "the transfusion of defibrinated blood has no influence in arresting transudation from bleeding surfaces, but is only useful in restoring the nutritive fluid when the drain has been stopped."

It has been proved by experiment that an animal cannot live without the supply of fresh nourishment by the alimentary canal. Blood transfused into the vessels does not take the place of food, and does not prevent an animal which is being starved from dying of inanition. The extra supply of blood corpuscles does not seem to serve as nutriment, but seems rather to a certain extent to be deleterious, by increasing the waste; apparently by using up an additional quantity of oxygen. Blood is regarded as a tissue which is used as the vehicle by which nourishment is carried to the other tissues, perhaps altering the constituents slightly in transit, but is not itself a source of nourishment. Neither is the fibrin used for purposes of nutrition. Even if it were the quantity is so small that it would not be worth while transfusing blood in order that its fibrin might be so used. In a pound of normal blood there is only about fifteen grains of fibrin. Fibrin is further of no essential service in carrying on the respiratory functions. Defibrinated blood is as capable of absorbing oxygen as that containing fibrin. "The one indication

for the use of transfusion is, therefore, deficiency of blood corpuscles, and the deficiency can be as well supplied by defibrinated as by fibrinous blood."

This remark of Professor Pannus, which I have just read, looks very plausible; but then what shall we say to the experiment of Brown-Séquard, who took ninety-five grams of blood from a dog, and replaced it with the same amount of milk. Shortly after the operation there was no trace of milk globules in the blood. Séquard claims the results are the same whether defibrinated blood, normal blood, or milk are used, but in the case of the milk, the quantity that it is necessary to inject is considerably more than in the others. I also find a case of exsanguination reported by J. C. Hutchinson, King's County Hospital, L. I., in which defibrinated blood was tried, and the patient grew worse for one week; when milk was transfused and the patient rapidly improved. The entire report of this case is very interesting, but it is too long to insert here. During the transfusion of the blood, which was done by Aveling's method, one of the joints accidentally became loosened, and a large amount of air entered the vein, but the patient suffered no inconvenience whatever therefrom.

Dr. Hutchinson thinks this danger has been somewhat over-rated, and that in the veins of an extremity certainly there was not the same peril accompanying the introduction of air as in the great veins of the neck or thorax. He also remarks that the milk ought always to be alkaline. It is, however, often acid. He renders it alkaline by adding one ounce of a weak solution of carbonate ammonia to eight ounces of milk. It has been proved by experiment that a dog which has apparently bled to death may be resuscitated by the transfusion of defibrinated blood, as well as by entire blood. The view of Magendie is, therefore, incorrect, that the presence of fibrin is of great importance for the passage of blood through the capillaries.

Experiment shows that when defibrinated blood is injected into the vessels of an animal of the same species the corpuscles are actually transplanted and survive in their new habitation. By using defibrinated blood you avoid the risk of embolism, which is considerable when entire blood is used. Dr. Roussel, of Geneva, thinks that the transfused blood should be from a similar origin, *i. e.*, blood from man to man, and venous blood into veins, etc.; also that it should not be altered in any of its constituents, that is, should not be defibrinated. Dr. Martin, of Berlin, advises the transfusion of pure human blood. Dr.

Athill, of Dublin, was in favor of using defibrinated blood, to avoid embolism and thrombosis.

I find, on reading up the subject, that most of the medical men advise the use of defibrinated blood, as its transfusion, as I previously stated, is attended with much less risk from embolism and thrombosis. Moreover, the beating up of the blood has a particular advantage in enabling the blood corpuscles to take up plenty of oxygen. The blood should be defibrinated by stirring with a wooden stick, and straining through a handkerchief. Should a second stirring produce more fibrin the straining should be repeated. The blood should stand in a vessel surrounded by water, at a temperature of 100° (38° C.).

The first operations of transfusion were by the direct method. In 1667, Albini, of Naples, performed transfusion, and the following description of the operation is in his own words: "Suppose a gum-elastic tube, about half a metre in length, to be inserted into the carotid artery of a living lamb, and placed in communication with a vein opened in the lady patient." Thus the heart of the lamb was used to propel the blood into the veins of the patient." On the 20th of November, 1669, Sauer and King performed the operation in England, on a man named Arthur Coyn, in the presence of many distinguished and intelligent people. The carotid of the lamb was prepared and a silver tube inserted from which the blood was allowed to flow freely into a vase. In one minute twelve ounces flowed out, and by this means they were able to approximate the quantity which flowed in a given time into the arm of the patient afterward. It is stated that the man derived great benefit from this operation, and that no evil consequences followed.

At the present time Aveling's method or some modification of it is used to a great extent, where it is desired to use pure blood without defibrination, and without doubt it is the simplest and much the best way of performing transfusion, where the blood is not to be defibrinated.

This can be very readily performed with a common Davidson syringe, by removing the pewter ends and substituting glass tubes. If you wish, the tubes can be heated in a spirit lamp, and bent so that they can be inserted more readily.

The india-rubber portion of the apparatus being filled with water, the operation is commenced by compressing the tube on the efferent side and squeezing the bulb; this forces two drachms of water into the afferent vein. Next

shift the hand and compress the tube on the afferent side; then allow the bulb to expand slowly, when blood will be drawn into it from the efferent vein, etc.

When it is desired to use defibrinated blood, which I think is much the safer method, we may proceed as follows: 1st. Expose the vein for about one inch of its course by dividing with scissors a transverse fold of skin pinched up between the fingers. About the vein thus exposed place three ligatures. The one at the distal end of the vein is to prevent loss of blood when the vein is opened, and is to be immediately tied, like an ordinary ligature.* The third on the proximal side is to guard against the introduction of air, and is only to be tightened. A valvular opening should now be made in the vein with the scissors, a little to the distal side of the middle ligature. Now we will suppose that the blood meanwhile has been drawn off and defibrinated as I directed before; so we fill a syringe with the blood, insert the nozzle in the opening in the vein, and inject enough to deprive this portion of the vein of any air it may contain; then tighten the middle ligature to secure the vessel to the syringe. Now the proximal ligature must be loosened and the piston of the syringe put in action.

This operation is simplicity itself, and well worth your attention, and if properly carried out it is an impossibility for it to be attended with any great amount of danger to your patient. This is Prof. Heuter's method of operation as described in the *British and Foreign Medico-Surgical Review*, only that he injects into the arteries instead of the veins. I have modified the description to make it apply to them, which you will very readily see only requires that the first ligature should be tied nearest to the heart, instead of on the distal side, as in the veins, and that the point of the syringe be directed toward the periphery instead of toward the heart; also, that the portion of the artery attached to the syringe should be cut out, to avoid aneurism. So you will observe that this method applies equally well, whether the injection be into a vein or an artery. Prof. Heuter thinks that injection into the arteries is far preferable to that in the veins. But it is found that in arterial transfusion a considerably greater amount of pressure is required than in venous transfusion. Thus in anæmia, that exerted by one hand will not suffice, and this is the great objection to its use. I find cases reported, where surgeons have tried arterial transfusion and could not make a success of the operation on that account, and then resorted

to venous transfusion with perfect success. Some have had a special syringe constructed, so that more pressure could be applied.

The most essential advantage of arterial transfusion over venous would seem to consist in the fact that the blood is conveyed to the heart more slowly and equally than in venous transfusion. And as the operation is usually resorted to only when life is fast ebbing away, and the heart's action at the lowest, it cannot be a matter of indifference to throw a quantity of blood suddenly into the right side of the heart.

Another advantage of arterial over venous transfusion is, that if a quantity of air gains admission, this does no mischief, but becomes absorbed; whereas, if it were to enter the vein, and ultimately the right side of the heart, it might act very mischievously. Again, the security from *phlebitis* has to be taken into account. A wound of the radial artery is less dangerous than one of the cephalic vein; for secondary hemorrhage from a properly secured artery is a rare occurrence, and it is less dangerous than the suppuration of a venous thrombi.

But whether venous or arterial transfusion is employed is a subordinate matter, as compared with the extension of the practice of the operation as a most powerful weapon in circumstances often regarded as hopeless.

In operating by Aveling's method, or direct transfusion, you will need no instruments besides those usually found in every doctor's pocket-case, except the syringe, which can be obtained at any drug store in the country.

By the indirect method, with defibrinated blood, you will need: (1) A spirit lamp, so arranged as to keep the blood at a temperature of 98° or thereabouts; two or three degrees above or below will do no harm. (2) A thermometer. (3) A good syringe. Any good pewter or hard-rubber syringe will do, provided the piston fits well. (4) A few instruments, such as scissors, forceps, etc. The best way is to set the dish containing the blood inside of another filled partially with water, in which the thermometer is placed, and all arranged or held over the lamp.

The whole operation can be performed with the hypodermic syringe; but that is pretty small, still the idea is worth remembering, as we always have that instrument with us. The idea occurred to me that the hypodermic syringe used by the veterinary surgeon would be very good, on account of its more suitable size.

As good an authority as Brown-Séquard says, that transfusion succeeds in animals when the blood made use of comes from a species of ani-

mal different from that of the one under experiment. But most writers on this subject maintain that the blood of a different species is very deleterious to the human system, if not actually poisonous, often producing dizziness, headache, cloudy urine, etc.

Human blood is far the best that can be used, and it certainly is always readily obtainable.

The danger of transfusion from one animal to another of a different species is due to the difference in the size of the corpuscles, thus stopping up the capillaries and obliterating them, causing grave accidents.

As to the quantity necessary in each case, no rule can be laid down. It will vary from one half ounce to twelve. It will be necessary to consider the condition of the patient, also to watch the effect of the operation as you proceed. Many severe cases are reported where only two, three or four ounces have been injected, and the patients recovered speedily without a repetition of the operation.

One would be naturally led to suppose that where one person gave a portion of his blood to another by transfusion, there would be no more danger to the donor than in any ordinary bleeding. But I find cases of death reported, two occurring in the last year, and the cases were all said to be conducted with skill and care.

Two doctors at St. Petersburg, appreciating that the great obstacle to successful transfusion was the coagulation of the blood transfused and the formation of thrombi, have instituted experiments to ascertain how far this may be avoided by employing the vessel of a living animal for a cannula. For this purpose a portion of the jugular or femoral vessels was cut from a living animal and passed into a glass tube a little shorter than the excised portion of the blood vessel, the ends of which were secured to the tube by silk threads. As a means of comparison a similar glass tube was employed without having the blood vessel attached to it. Another plan was to dissect out a large portion of the blood vessel, and only to arm the free ends with the glass tube. This last plan being most suitable when venous blood is employed. The experiments showed that while blood on entering the simple glass tube immediately formed small coagula, that which passed through the living cannula remained fluid; provided that no lesion of the inner wall had taken place. The blood vessels thus removed from the animals retained their power of maintaining the fluidity of the blood for one hour and a

half. During the experiment the stream was often interrupted and then allowed to flow again, without any coagulation being induced; while the whole calibre of the glass tube became blocked up in seven minutes."—*Med. and Surg. Reporter, March 3d, 1877.*

This last idea I introduce thinking that it may suggest some idea that may be of service. But what I have endeavored to impress upon the practitioner's mind all the way through this article, is the comparative simplicity of this operation, and that all the necessary instruments for the successful performance of the same can be found in every wide-awake practitioner's office. And further that we have in this operation a most powerful weapon in circumstances often regarded as hopeless.

HOSPITAL REPORTS.

NEW HAVEN (CONN.) GENERAL HOSPITAL.

Prevailing Diseases and their Treatment, with Report of Cases.

Our hospital is, as you know, one of the smaller hospitals, ranking with those at Hartford, Worcester, etc. The average number of patients varies between 75 and 80. Not being an endowed institution, it has to depend largely for its support on the board of the patients. These may be divided into four classes, *i. e.*, soldier, sailor, town, and self-paying patients.

The most common diseases which are met with are these: (1) phthisis (chronic catarrhal pneumonia), forming a large proportion of all the medical cases, and presenting no peculiarities whatever.

The next most common are cases of alcoholism, of which we have a large number each year, and in whose treatment we have been very successful.

Next to these come cases of intermittent fever, which are very common, the disease being apparently contracted in this neighborhood. It is not an unfrequent occurrence to see persons who have never been outside of this State having their regular morning shake.

Next in point of numbers would come the confinement cases, and cases of chronic rheumatism.

Of the surgical cases, those of fracture are by far the most common. Then come cases of coxæ morbus; of these we have had a number of chronic cases, which have been operated upon again and again without any permanent benefit. Cases of stricture are quite common, but so far they have presented little of interest.

There were three cases of ovarian tumors, in which operations (without Lister) met with great success. One of these cases was most unpromising, but the result was highly satisfactory.

So far as treatment goes, it is very difficult to say really how a given case would be treated, for the treatment depends entirely upon the visiting

physician, and so the cases are treated in various ways.

As a rule the treatment is simple, single drugs being used in most cases, and not elaborate and intricate prescriptions.

The cases, so far as I have seen, do remarkably well.

In cases of acute rheumatism the routine treatment is—

R. Salicine, gr. xx.

In wafers every two hours,

combined with external bathing of the parts with an alkaline wash, and wrapping the afflicted joints in cotton batting.

The salicine has been found to answer all the purposes of salicylic acid, and is much less irritating to the stomach and less likely to produce nausea, and furthermore, is very much cheaper.

As to house prescriptions we have very few, but of them one is used almost constantly as a stimulant and febrifuge:—

R. Camphoræ, ʒj
Ammon. carb., ʒj
Muc. acaciæ, ʒj
Aque, ʒj. M.

Sig.—ʒ ss every two hours.

The lietus morphia is also much used. The formula for it is as follows:—

R. Morphia sulph. gr. j
Spts. chloroform, ʒj
Muc. gm. acaciæ, ʒj
Syr. tolu. q. s. ut. ft. ʒjss. M.

A favorite diarrhoea mixture is as follows:—

R. Tr. opii,
Tr. capsici,
Tr. rhei. aromat.,
Spts. camphoræ,
Spts. menth. pip., aa.

We have of late used quite extensively codeia, as a substitute for morphia, and with very good results. It is given in capsules, and can be taken by those who are unable to take morphia in any form.

It is, of course, no new thing to give hypodermic injections of water for pain; but a more curious case than this came to our notice lately. A weak, anæmic woman, forty-five years of age, with chronic gastritis and several other chronic affections, had been in the habit of taking morphia in the form of "sleeping drops," and for the first few nights she was given morphia. But after this we had a bottle carefully labeled "sleeping drops," and filled with water. Of this she took a drachm at bedtime. At first she complained that the medicine was too strong and made her head feel badly the next day; so that the dose was reduced one half. This she took for nearly a month, sleeping nicely when she did take it, and not sleeping at all when it was withheld.

Our treatment of acute pulmonary diseases consists almost entirely in the external application of large and frequently renewed flaxseed poultices, covering the back and front of the chest.

The general treatment of all the diseases here is almost entirely symptomatic.

As regards bedding, we have nothing peculiar, with the exception of a water bed, which I be-

lieve was manufactured here. It consists of a series of separate rubber bags, long and narrow, which are placed crosswise of the bed, and can be filled just as the case requires. This, when nearly filled, makes a very comfortable bed, and the pressure of any part of it can easily be regulated.

In the surgical wards few new inventions are in use. The plaster-of-Paris bandages for fractures are largely used, and especially the Bavarian splint. In applying this we have found that the two layers of flannel, with the plaster between, does not make a stiff enough bandage for fractures, so now we place five or six layers of crinoline, carefully cut in the shape of the limb, between the two coverings, and firmly imbed them in the plaster. In this way we get a very reliable splint.

In the dressing of ulcers we use largely a mixture of one part of iodoform and three parts clay; this makes an excellent dressing, and has been found by us to be superior to simple iodoform. Salicylic cotton and vaseline have been largely used, when partial Lister dressings have been desired, with very good results.

A saturated solution of tannin in collodion is used as an application to check hemorrhage, as in cases of carcinoma uteri, etc.

Interesting Cases.

John O'W., aged 49 years, came to the hospital with a note from a doctor who wished us to operate for strangulated hernia. The man was apparently much prostrated and walked with great difficulty. On inquiring into the history of the case we learned that Sunday before entrance, while sitting on a board pile, he fell and struck on the sharp edge of a plank, receiving the full force of the fall on his scrotum. This immediately began to swell and to be discolored. On entrance, the scrotum was fully the size of a child's head, greatly discolored and cold. There seemed to be a partial line of demarcation formed.

The scrotum was carefully washed, wrapped in cotton and elevated, and stimulants and tonics given. The next day the line seemed to have disappeared, the scrotum was warm, and all seemed favorable. In a day or two the scrotum became cold again, and the whole lower part began to slough. Charcoal poultices were applied and the stimulants increased, the man's strength being quite good. In three days the slough came away, leaving both testicles entirely exposed. A most obstinate diarrhoea set in, but was finally controlled by suppositories of tannin and opium. The edges of the wound began to granulate, and now the entire scrotum is nearly replaced and the patient is in very good condition.

Henry B., forty-eight years old, married, and a cook by trade, entered hospital for paralysis, July 18th, 1879. Improved rapidly until August 20th. when suddenly the patient began to complain of severe headache, with loss of appetite, and constipation. At 2 A.M. of the 21st he was found sitting upon the bed and complaining greatly of those severe pains in his head. Temperature found to be 96° in axilla, pulse soft and compressible, 66 per minute, heart's action weak

and irregular. Patient sank rapidly, becoming too weak to sit up, and complained of vertigo if his head was raised in the least. The tongue soon became paralyzed, so that speech was impossible and swallowing performed with great difficulty. Great pain in spine and through abdomen. Entire spine tender on pressure and pain greatly increased by motion. Respiration at times rapid and shallow, but generally slow and irregular, often not more than three per minute by actual count. Whisky, $\bar{3}$ j. with aqua ammonia, gtt. xx, was given at first, followed by $\bar{3}$ as dose of first with same dose of ammonia every half hour, later every hour. Heart's action much improved; frequent spasmodic attacks, with great pain, during which muscles were in a state of tetanic contraction, and once opisthotonos to a marked degree was produced.

At 7.30 A.M. twenty grains of quinia was given, and later a blister was applied to the nucha, after which the patient gradually regained power of speech.

During the day was quite comfortable. At 6 P.M. breathing began to grow irregular and infrequent, as during preceding night. At 7 P.M. was attacked with violent nausea and vomiting, followed by fainting fits. Subcutaneous injections of dilute aq. ammonia and whisky were given; patient recovered some and slept the rest of the night. The next night the patient had a similar attack, but with this there was slight delirium. The temperature was remarkably low, but the body throughout the attacks felt warm, and was at times covered with perspiration. Hyperæsthesia over abdomen and legs below the knees were insensible to pain, and during one attack, the arm also. Constant rolling of the head on the pillow.

On the 30th instant patient had entirely recovered, and nothing of this kind ever occurred again during his stay in the hospital.

Charles G., twenty-four years; Irish. Cause of death, acute catarrhal pneumonia. Autopsy by Dr. W. H. Carmalt. This case presented one feature which was to us very interesting, i.e., the kidneys; these were united by their inferior extremities, in the form of a horseshoe, riding the spine like a saddle. The renal arteries arose from the aorta by a common trunk or axis, about one inch in length; this trunk divided in two branches, which entered the kidneys at each hilus. The ureters arose from the anterior and inferior portion of pelvis, and passed downward in front.

The substance of the kidney was uniformly healthy. No urinary symptoms during life.

Isaac Pardu, sixty years; American. Pernicious anæmia. Began to lose flesh and strength gradually two years before entrance, without any apparent cause. He had no sickness aside from this. This condition of things continued up to the time of his death. His appetite was enormous. Urine low specific gravity, but no sugar or albumen.

The post-mortem revealed a curious condition of things. There was a general softened condition of the whole bony system; both humeri were broken; the ribs were so soft that the least pressure caused a fracture. The blood was thin and pink in color, and did not coagulate. The muscular tissues were very friable and pale in color.

Aside from this nothing was found. The thoracic duct was pervious.

Some little time before entrance he received a blow on the head, which caused a considerable swelling; this proved to be a hæmatoma, which, by its pressure, and by the softened condition of the bones of the skull, had eaten its way through both tables and was pressing directly on the dura mater.

EDITORIAL DEPARTMENT.

PERISCOPE.

Treatment of Scarlet Fever by Warm Baths.

The following communication from W. Vawdrey Lush, M.D., physician to the Dorset County Hospital, appeared in *The Lancet*, August 14th, 1880:—

In December, 1869, while we were experiencing a very severe epidemic of scarlet fever, there appeared in *The Lancet* a reprint of a letter by Dr. Charles T. Thompson, strongly advocating the use of warm baths in this disease, and stating that he had pursued the practice for fifteen years, and had never lost a patient.

In consequence of this communication, I commenced this practice ten years ago, and have followed it from that time to the present. At first I order the patient to have three warm baths daily, to be kept in from three to five minutes, rapidly dried, wrapped in a blanket, and returned

to bed. As the disease subsides, I reduce the baths to two or only one daily. I find that—1st, it brings out the rash; 2d, reduces the temperature; 3d, soothes the patient; and when this treatment has been adopted at the onset, I have as yet not lost a single patient.

In one case the warm bath was objected to till the child had been ill some days, and this case, and this alone, proved fatal.

My friend, Dr. Alfred Hollis, of Freetown, has told me of the great comfort he himself experienced from warm bathing when suffering from the disease; and, of course, in the treatment neither medicine proper nor good nursing is precluded.

Some of my readers may recollect a case of smallpox published by the late Dr. Stokes, of Dublin, where the warm bath proved singularly beneficial, and who doubted not that the mortality in smallpox hospitals would be greatly diminished by the use of the bath. The case I

refer to was that of a medical student, in which "the pustulation was almost universally confluent; the purulent matter highly putrescent; the hemorrhagic state developed, the body one universal ulcerous sore, and the blackness of the worst purpura developed; the odor of an intensely pungent and offensive character, which seemed to pass through the bystander like a sword. Stimulants alone, freely and constantly employed, seemed to preserve the patient alive. The pulse was rapid, weak, and intermittent, and for several days life was despaired of. At this juncture Dr. Stokes happened to describe the case to his colleague, Mr. Smyly, who suggested the trial of the warm bath. Pillows were adjusted in one, the patient placed in it, and the effect was instantaneous and marvelous. The delirium immediately ceased. The patient exclaimed, 'I am in heaven! I am in heaven! Why didn't you do this before?' He was kept at least seven hours in the bath, brandy being freely administered, and removed to bed. The bath was repeated next day, after which he fell for the first time into a tranquil slumber. From this time recovery was progressive."

This may seem a digression; but the treatment of another of the exanthemata by similar means is not inapposite.

My ten years added to Dr. Thompson's fifteen make twenty-five years' experience of a treatment which I can confidently and heartily recommend.

Chloroform in the Treatment of Puerperal Convulsions.

Dr. K. N. Fenwick, of Kingston, Ont., reports the following interesting case in the *Canada Lancet*, September 1st, 1880:—

Mrs. L., aged twenty-one, primipara, dress-maker, had always enjoyed good health, and, until a few weeks before delivery had been very well, with the exception of occasional attacks of facial neuralgia. During the last few weeks of her pregnancy she had swelling of the face and feet. On the afternoon of the 10th of September, 1879, she complained of very severe headache, of a different character from her former neuralgia, accompanied by a slight diarrhoea. I was first called to see her upon the same day, about 9.30 P.M., when she had a convulsion. She had slight regular pains in the back, and on examining the os, found it hardly dilated enough to admit the index finger, the head presenting in a roomy pelvis. I made her inhale chloroform with the effect of arresting the convulsions, and then gave potassium bromide, grs. xv, every hour, with the effect of warding off the attacks until 5 A.M.

As the convulsions now returned with greater frequency, the os still undilated any more than on my first examination, and the patient becoming comatose, on consultation with Dr. Lavell, decided to deliver at once. I then directed a stream of hot water against the os, by means of a ball syringe, and began to forcibly dilate the os with the fingers, repeating the hot water injections, and carefully dilating the os until in less than half an hour it was fully open. I then ruptured the membranes, gave a dose of ergot,

drew off a few ounces of highly albuminous urine, applied the forceps and delivered a healthy living child. Chloroform was administered during the forcible dilatation and application of forceps, with the effect of modifying, though not preventing the convulsions. The uterus contracted well and no hemorrhage occurred. She had two convulsions after delivery, but they were slight, were at once checked by chloroform, and never returned. Gave chloral, grs. xxx, by rectum, every four hours.

At 9 P.M. temperature was 100° F. Though insensible she took beef tea from a spoon with avidity. I then gave a diuretic mixture of acetate of potash, digitalis and broom. Sensibility slowly and gradually returned, until, on the 13th, she asked to see some of the family. From this time her recovery was rapid, and to-day (June 8th, 1880,) both mother and child are in perfect health.

The interesting point about the case was the rapidity with which the os uteri became dilated under the influence of the hot water injections, and digital manipulation. Some authorities, such as Leishman, are averse to forcible dilatation during puerperal convulsions, on the ground that it increases the attacks; but there can be little doubt, from the duration of the comatose symptoms, that an expectant treatment in this case would have ended fatally.

Secondary Syphilitic Epilepsy.

M. Fournier states, in *Annales de Dermatologie et de Syphiligraphie*, Nos. 1 and 2, 1880, that his object in publishing the following cases is to establish the fact that certain symptoms of an epileptic character do sometimes occur during the secondary stage of syphilis, and especially during the early part of this stage. The symptoms are very different from those which occur during the tertiary period, as regards significance, evolution, and gravity.

CASE 1.—A young woman of fair constitution and previous health, was admitted into the Lourcine Hospital on account of secondary lesions of the vulva and mouth. Some weeks later there occurred a severe outbreak of cutaneous syphilis, with enlargement of the glands, affections of the periosteum, headache, insomnia, etc. Suddenly there appeared a series of peculiar attacks, some of which were witnessed by M. Fournier, and of which he gives the following description. "The countenance suddenly assumes an extraordinary indescribably stupid expression. The head is turned from the left side. The left upper extremity is suddenly raised toward the face, the wrist and fingers being strongly flexed. The lower limb of the same side is forcibly extended. The whole body, in short, is rotated from right to left. The phenomenon lasts a few seconds; then violent clonic spasms affect the left upper limb. Finally, the patient for some minutes longer remains stupid, stunned, and unconscious, and the fit is at an end." During several days these fits were repeated, but very soon there appeared attacks of general convulsive epilepsy, in every respect resembling the *haut mal*. There were a large number of such attacks during the first month. In the second month there were

five fits, and only two during the next two months. Under energetic specific treatment the cutaneous and other lesions disappeared; the epileptic attacks also ceased and did not return.

CASE 2.—A strong young woman, of good constitution, was found on admission to be suffering from chancrous induration of the labium, with inguinal adenopathy, roseola, lesions of the tonsils, and general analgesia of the skin and mucous membranes. She had not undergone any treatment previously. Some days after admission the patient was seized during the night with a violent convulsive attack. She was found to be unconscious, foaming at the mouth, and in a state of convulsion like that of the *haut mal*. After the fit she fell into a deep sleep. A second fit occurred about a quarter of an hour later, followed by involuntary evacuation of the bladder, and an almost comatose condition, which lasted until the following morning. The next day M. Fournier found the patient somnolent, and incapable of answering questions. The excitement of the examination, however, brought on another fit, which was carefully observed by M. Fournier, and found to present all the characters of ordinary epilepsy. The next day consciousness was fully regained, and the patient then affirmed that she had never before suffered from any attack of the kind, nor from any nervous affection whatever. There was no family history of any nervous disorder. Minute examination failed to reveal any other cause than syphilis. Protoiodide of mercury had already been prescribed, and was continued without any other medication; the effect being that the roseola and other signs of syphilis quickly disappeared, and the fits did not return. Three months later the patient was discharged in good health, but was seen on several occasions during a year afterwards for various slight syphilitic symptoms. There had been no return of the epileptic seizures.

CASE 3.—A robust young man, aged 25, of nervous temperament, contracted syphilis in 1867. In the secondary period, between the third and sixth months, he had three well-marked attacks of epilepsy. There was no hereditary tendency, and no other cause except syphilis could be ascertained. Under specific treatment the external signs of syphilis disappeared, and there was no repetition of the epileptic attacks. Thirteen years have now passed without any re-appearance. These three cases, with about a dozen others observed by the author, prove, in M. Fournier's opinion, that symptoms of the nature of epilepsy may occur during the first few months of syphilis. M. Fournier looks upon this secondary epilepsy as being a *specific neurosis*; that is to say, it consists of a group of morbid symptoms developed under a specific influence, but occurring probably without any lesion of the nervous centres.

Traumatic Tetanus Treated by Stretching the Median Nerve.

The following case, which is reported in the *Medical Times and Gazette*, August 21st, 1880, occurred in the Seamen's Hospital, Greenwich, under the care of Mr. W. Johnson Smith:—

R. B., aged fifty-four, a spare and feeble man,

was admitted on the morning of January 23d, 1880, with injuries to the right forearm and left thigh, caused by the sinking of the roots and of the lower portion of the trunk of an old elm tree which he had been attempting, at the bottom of a deep excavation, to undermine. On examination there was found a simple transverse fracture of the shaft of the left femur, near the junction of the lower with the middle third. There was marked over-riding of the upper fragment, and also considerable eversion of the foot. On the posterior surface of the left forearm was an extensive oval wound measuring eight inches in the vertical direction and three inches from side to side. Both the skin and deep fasciæ had been torn through, and the tendons of the extensor muscles were freely exposed. The skin beyond the margins of the open wound was extensively undermined, and much of the raw surface was covered by damp earth. No portion of either of the bones seemed to have been stripped of soft covering.

The injured lower limb on the left side was suspended in Hodgen's apparatus, and it was retained in this until February 23d.

On the third day after admission the right forearm, which had been kept in a constant bath containing a very weak solution of carbolic acid, became the seat of suppurative cellulitis, which spread rapidly to the front of the limb and upward beyond the elbow. Free incisions were made on the following day in various parts of the swollen extremity, and one of these was carried through a gangrenous patch of skin in front of the elbow and to the inner side of the tendon of the biceps.

During the first week of February, while the swelling of the forearm was subsiding and an extensive granulating surface was being rapidly cleared of large masses of slough, the patient, whose general health had remained good, complained very much of painful contractions of the flexor muscles of the forearm and of forcible flexion of the fingers, the tips of which were from time to time tightly pressed into the palm.

This condition of spasm increased very much in severity during the second week, and prevented the patient from sleeping. On February 12th, after a sudden rise in his temperature, he complained of stiff neck and difficulty in swallowing. On the following morning there was well marked risus sardonius; the lower jaw was tightly set, the abdominal muscles and those of the lower extremities were from time to time very hard, and there was acute pain in the epigastrium. At noon, ether having been administered, the median nerve was exposed in the middle of the arm, and then, after it had been compressed, was forcibly dragged both downward and upward. The wound was made under the spray (carbolic acid), and dressed with gauze.

After the effects of the anæsthetic had passed off, the patient found that the right forearm was quite free from pain, and that the spasm of the flexor muscles had ceased. The fingers were quite lax, and could be extended without difficulty. This improvement seemed to be permanent, as no recurrence of the involuntary mus-

cular contraction was complained of during the slow healing of the extensive open surface on the back of the limb. On the evening of the 13th (the date of operation) he still complained of difficulty of swallowing and of pain and stiffness in the abdominal muscles, but in the course of the two following days these symptoms gradually subsided. In consequence of retarded union at the seat of fracture of the left femur, the patient was unable to leave his bed before the end of April. He was discharged as convalescent on May 10th, being then able to walk with the support of a stout stick, and with the wound of the forearm almost quite closed, the functions of this extremity, however, being much impaired through the extent of the cicatrization and consequent constriction of all the soft parts.

REVIEWS AND BOOK NOTICES.

NOTES ON CURRENT MEDICAL LITERATURE.

—"Pregnancy Vomiting," is the title of a pamphlet by J. Marion Sims, M.D., reprinted from the *Archives of Medicine*, June, 1880.

—A reprint from the *Archives of Ophthalmology*, June, 1880, contains a paper on the use of "Salicylate of Sodium in the Treatment of Iritis," by Julian J. Chisolm, M.D.

—Dr. Webb J. Kelly, of Galion, Ohio, sends us a pamphlet containing "A Summary of Eight Hundred Obstetrical Cases," occurring in the practice of H. R. Kelly, M.D., Galion, Ohio.

—"The Mechanical Treatment of Cystocele and Procidentia Uteri," is the subject of a paper by Eugene C. Gehrung, M.D., of St. Louis, which comes to us in the form of a reprint from the *American Journal of Obstetrics and Diseases of Women*, for July, 1880.

—A reprint from *Transactions Missouri State Medical Society*, 1880, contains remarks on "Abdominal Surgery, Cyst of Broad Ligament, Laparotomy, Recovery," by F. J. Lutz, A.M., M.D., of St. Louis, Mo.

BOOK NOTICES.

What to Do First, in Accidents or Poisoning. By Charles W. Dulles, M.D., Surgical Registrar to the Hospital of the University of Pennsylvania, etc. Philadelphia, Presley Blakiston, 1012 Walnut Street, 1880. Cloth, 16mo. pp. 64. Price 50 cents.

The title of this work sufficiently indicates its character. It is intended to serve as a guide to those who may happen to be present at an accident, and to point out to them what to do until the arrival of a physician. The usefulness of a

work of this kind can never be over-estimated, for a certain amount of knowledge of what is needed in an emergency will keep its possessor cool, where ignorance will only produce confusion. Only accidents of common occurrence, such as drowning, hanging, suffocation with noxious gases, fits, freezing, burns, sunstroke, sprains, fractures and dislocations, wounds of various kinds, hemorrhage, railroad and machinery accidents, with transportation of injured persons, and poisoning are treated of. The book is furnished with numerous cuts and its value is increased by the addition of a complete index. We cheerfully recommend this little work to the intelligent public.

The Pathology, Diagnosis, and Treatment of Diseases of Women, including the Diagnosis of Pregnancy. By Graily Hewitt, M.D., London, F.R.C.P.; Professor of Midwifery and Diseases of Women, University College, and Obstetric Physician to the Hospital, etc., etc. Third American, from the Third London Edition, Revised and Enlarged. With one hundred and thirty-two illustrations. Philadelphia, Lindsay & Blakiston, 1880. 8vo., pp. 761. Price, cloth, \$4.00; leather, \$5.00.

The author informs us, in the preface to this edition, that while the former editions contained, the first, mainly a summary and a criticism of the then existing knowledge on the subject of the diseases of women, and the second, numerous pictorial illustrations of his own observations, with suggestions of an improved system of uterine pathology, this, the third edition, contains certain generalizations on the important question of the pathology of diseases of the uterus which have forced themselves on his attention in the course of several years' experience, and which involve the adoption of views in reference to the pathology and treatment of those diseases which are new, as compared with those embodied in the early editions of this work. The results of a series of observations made by the author in the University College Hospital, during a period of four years, are given in support of the doctrines advanced. The cases are carefully tabulated, and long histories of the same dispensed with. The various subjects treated of are systematically arranged, and, it seems to us, exceedingly well elucidated, both as regards diagnosis and treatment, and although several excellent works on gynecology have recently been published, we feel sure that no one will consider this addition to the literature as superfluous. The book is gotten up in the usual elegant style of the publishers.

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D. G. BRINTON, M.D., EDITOR.

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THE INVIOABILITY OF PROFESSIONAL COMMUNICATIONS.

It is indispensable to the proper treatment of a case that the attending physician be made acquainted with its history. This frequently involves disclosures on the part of the patient which no other consideration than the risk of his own life would induce him to make. Not rarely, they are of such a nature that they would not only blast his reputation were they generally known, but would criminate him, were they made in a court of justice. As such, the law would sanction his refusal to make them, even in the pursuit of the ends of public justice, and to secure the conviction of dangerous malefactors. Is not the physician bound, by every tie of honor and duty, to refuse to reveal such professional confidences, and should not the law protect him to the utmost in this refusal?

We believe that it should, in the fullest measure. Yet the matter is not decided in many States. There are still instances, from time to time, where a physician is called to the witness

stand, and the court permits him to be cross-questioned as to professional confidences, and will not admit the plea that he is not permitted to repeat them. In some States, indeed, we believe it has been decided that the physician must testify to the best of his knowledge and belief, or expose himself to the penalties of contempt of court. The law of New York State is more enlightened on this subject. It is given as a sort of *obiter dictum*, in an address on malpractice, last winter, by GEORGE CLINTON, L.L.D. late Chief Justice of the Supreme Court of the City of Buffalo.

That learned authority justly states that at common law the disclosures of the patient to his physician are not sacred; and the physician, as a witness, is compelled to testify as to the communications of the patient, however necessary to enable him to judge and prescribe. The Revised Statutes of New York contain an enactment, which, as re-enacted by the code, with some change of phraseology, reads thus: "A person duly authorized to practice physic or surgery shall not be allowed to disclose any information which he has acquired in attending a patient in a professional capacity, and which was necessary to enable him to act in that capacity." (Code, § 832). This language is extremely broad and comprehensive. Judge CLINTON believes that the courts will construe it as applying only to information derived from the patient, and apply it only to actions and indictments in which he is directly interested. Surely, if a man be injured by another, the physician who attended him must be admitted, on the trial of the injurer, to prove all of the injured man's disclosures, and his bodily condition and symptoms. In *Johnson vs. Johnson* (4 Paige, 460), Chancellor Walworth held that the testimony of the physician was admissible, he only objecting. But upon the reversal of that case in the Court for the Correction of Errors, Chief Justice Savage maintained what is apparently the true doctrine, that the secrecy of the physician is solely the patient's privilege, and that the physician cannot testify as to the facts within the Statute prohibition unless the patient grants permission. This view of the

case recommends itself to common sense as well as to sound law.

The New York Statutes not only protect the physician if he declines to testify without the patient's consent, but they also are framed to punish the physician should he make damaging disclosures without the patient's permission. This is as it should be, for if honor does not seal the medical attendant's lips, then it is well that the law shall clasp its iron padlock on them.

It were highly desirable that these questions were settled in all the States. The welfare of the profession is so closely interested that medical societies generally should take up the subject and have Acts passed similar to those which exist in New York, and which, so far as we can learn, have worked very well. The field of medico-legal studies might well be extended to the more exact defining of the relations of physicians to their patients on the one hand, and to courts of justice on the other.

NOTES AND COMMENTS.

Therapeutical Notes.

SALICYLATED CAMPHOR IN PHAGEDENIC ULCERATION.

In the treatment of this form of ulceration, Dr. Lajoue, according to *La France Médicale*, employs an ointment composed as follows: camphor, 10 grams; salicylic acid, 10 grams. These are mixed, and then there is added, alcohol (at 90°C.), 10 drops; vaseline, 10 grams; paraffine, 25 grams. This makes a consistent ointment, which is put upon the wounds.

PEPTOGENIC ELIXIR.

In cases of dyspepsia where the indications are to favor the secretion of gastric juice, and to introduce peptogenic substances into the stomach, Dr. Dujardin-Beaumetz employs an elixir, the formula of which, as published in *La France Médicale*, is as follows:—dextrine, 10 parts; rum, 20 parts; syrup, 60; water, 120. This elixir has a very agreeable taste, and is of service in the cure of atonic and putrid dyspepsia.

Bathing after Meals.

Two cases carefully studied by Dr. Naegli, and published in the *Swiss Medical Journal*, afford a striking illustration of the old warning not to

bathe with a full stomach. The cases were those of two persons who died when bathing, and in whom the post-mortem examinations revealed similar appearances, affording a certain evidence that death was due to the fullness of the stomach with food at the period of immersion. The first is the case of a lad, fourteen years old, who, on a holiday, after regaling himself with bread and sausages, and a glass of beer, went into the river, with a comrade, for a swim, in the evening, and was swimming gaily ahead of his friends, when suddenly, uttering only an indistinct sound, he sank below the surface; assistance was quickly at hand, and he was drawn out within three or four minutes after he had sunk. The physician was at hand at once, and the fullest hopes were entertained of being able to restore life, as the period of immersion had been so short and the heart could still be heard to beat. Artificial respiration was at once employed, but without any success; then, without much delay, tracheotomy was had recourse to; but on opening the larynx, instead of air rushing out with the well-known sound, a stream of fluid food escaped from the wound. Every means was now attempted to remove by suction these foreign substances from the obstructed trachea, but uselessly; and, in spite of all endeavors, the lad could not be restored to life. A post-mortem examination showed but few of the usual signs of suffocation from drowning, but the trachea was injected rosy red, contained small quantities of food, and the larger bronchi and bronchioles showed, in their lumen, small pieces of potato entangled and obstructed; even sections in the periphery of the congested lung showed the remains of food in the larger bronchioles. The second case was one of a student aged eighteen, who went to bathe after an evening meal consisting of cheese, bread, and beer; and soon after entering the bath, sank suddenly, uttering a dull, gurgling sound, and was only recovered from the water a quarter of an hour afterwards, being then quite dead and incapable of being restored to life. A post-mortem examination showed, again, the larynx and air-tubes full of fluid contents of the stomach, the bronchi and larger bronchioles extending into the lung being obstructed by small particles of cheese. These two post-mortem examinations show that both of these unfortunate persons were suffocated by the food which had passed into their trachea and lungs when sinking; the explanation appears to be that the bathers had entered the water with a full stomach; that the pressure of the water on the abdomen, and the efforts in swimming, which not unfrequently pro-

duce in swimmers a slight feeling of sea-sickness, had induced nausea and vomiting. This had suddenly taken away their strength; they had sunk under the water, and in sinking had drawn in the food thrown up from the stomach and water into the larynx: the trachea had thus become obstructed by food; so that, even in the case of immediate help, which in this case was at hand, recovery could not be effected. The warning "not to bathe when the stomach is full of food" is certainly very strongly emphasized in these carefully observed and conclusive cases.

Color-Blindness.

The *British Medical Journal*, August 21, 1880, informs us that Dr. Favre, of Lyons, recently read a paper of much general interest on the relations of color-blindness, at the Paris Academy of Medicine. He has examined more than ten thousand male adults, by different methods, and has discovered that more than ten per cent. of them were not capable of distinguishing one or several of the five elementary colors; he has also met with two cases of serious and comparatively dangerous Daltonism, and eight cases of injurious or troublesome chromatopseudopsia. The examinations made in reference to railroads, the army and navy, would be insufficient if it were necessary to determine by the examination of the sense of color the aptitude of individuals for those commercial or industrial pursuits which involve the examination of colored objects. It would, perhaps, be necessary, in that case, to examine two hundred or three hundred shades and colors. Among the very numerous mistakes quoted by Dr. Favre, several were made by woolen-drapers, tailors, jewelers, weavers, and dyers. An exact knowledge of colors is necessary to magistrates, and is indispensable to experts, who should have a very acute sense of color. Dr. Favre quotes instances in which M. Ferraud, an analytical chemist, formerly assistant to M. Chevreul at the Gobelins, has been able to rectify in his reports very important errors committed with regard to colors in the description of inculpatory objects in criminal cases. The reports had been drawn up by rural constables, policemen, or other agents of the authority. Chemists, botanists, and micrographists are often very much troubled. General practitioners and chemists suffering from dyschromatopsia know very well generally how to manage matters; they know how to make use of the persons whose sight is normal. M. Favre has sought out the cases of dyschromatopsia which have been publicly re-

corded, and especially those which have come before the tribunals of justice. Discussions and quarrels have occurred in schools; some cases have been brought before the tribunals. The examination of the color-sense of the persons at issue would be sufficient to enlighten the judges. The errors of Daltonians with regard to postage-stamps have been so frequent as to determine the French authorities to increase the diameter of the figures, and to exact an examination in colors from their staff. All these circumstances go to prove that such an examination should be more generally resorted to, and that the color-blind should be made responsible to the law for their mistakes; and, as Daltonism can for the most part be cured by exercise, the legislation required would certainly become an excellent therapeutic agent. Those persons who were past cure would be warned that they must abstain from giving any judgments on colored objects.

Diseases of the Uveal Tract.

In an able paper in Knapp's *Archives of Ophthalmology*, vol. x, No. 2, June, 1880, Dr. Max Knies relates the clinical history and post-mortem examination of a case of double serous iritis. The patient was aged nineteen, and while under treatment died somewhat suddenly, from croupous laryngitis complicated with pneumonia. The eyes were removed shortly after death and preserved in Müller's fluid, and when sufficiently hardened were submitted to very careful microscopic examination. The centre of inflammation appeared to lie at the attachment of the iris and ciliary muscle, and in the adjoining portions of the ciliary body. Bowman's membrane was thickened, which the author believes points to the existence of 'a process located in the anterior part of the globe' and connected with increased tissue changes. In the formation of the patches characteristic of this affection the membrane of Descemet is probably quite passive. A very important point brought to light by the post-mortem examination was the pronounced cellular infiltration of the pia sheath of pia mater from the optic disc to the chiasma; hence, the author concludes that serous iritis is continuous and connected in both eyes. This condition of the optic nerve he regards as independent of any cerebral affection, as peculiar to iritis serosa, and as forming the connection between the affection of one eye and of the other. Iritis serosa, according to the author, is essentially an affection of the whole globe, with the exception of the lens, which,

however, frequently becomes affected in later stages of the disease. The seat of the inflammation is primarily in the uvea; but its continuous propagation can be demonstrated through the choroid, papilla, and optic nerve, along the chiasma, to the other eye, a process which hitherto has only been known to exist in certain gliomata. Iritis serosa has this in common with glaucoma, that the region of insertion of the iris participates in the affection, and that great variations of tension are often observed. Accurately speaking, iritis serosa is really an uveitis serosa in the widest sense of the word, in opposition to the purulent form; between these, however, only quantitative differences exist. The anatomical proof of a direct continuity in the inflammation of both eyes, as established by post-mortem examination in this case, is of fundamental importance, in explaining the origin of sympathetic affections. It would rehabilitate Mackenzie's theory of the propagation of sympathetic ophthalmia by means of the optic nerves through the chiasma. Should subsequent researches demonstrate the correctness of the inferences drawn from this case, it would no longer be possible to refuse unconditionally to accept a continuity in other double uveal affections, which begin with neuritis and do not occur simultaneously.

The Pathology of Lead Palsy.

Zunker relates, in *Zeitschrift für Klinische Medicin*, the case of a painter who suffered from lead colic and paralysis, advanced phthisis, and chronic renal disease. He had marked blue line on gums, double paralysis and atrophy of the extensors of the hand and fingers (the supinators excepted), atrophy of the thenar, hypothenar, and interosseus muscles, paralysis of the extensores digit., pedis, and peronii on the left, paresis of these muscles on the right side, no alterations of sensation, bladder and rectum intact, mind unaffected, the paralyzed muscles not excited by either current, degenerative reaction of the paretic leg muscles on the right side. He found, post-mortem, corpora amylacea in the anterior and postal spinal nerve roots, as well as on the cord itself, especially in the posterior columns. The fibres of the nerve roots were only smaller than normal. In the cervical region the gray matter was of a normal shape; in the dorsal region, from the middle downward, the left anterior columns were smaller, atrophied, and full of corpora amylacea, finely fibrillated; the anterior horn cells

were smaller than those of the opposite, in part completely disappeared. In the cervical and lumbar regions the multipolar ganglion cells were smaller and darker than normal, and destitute of broad processes, but not to be compared with the atrophy seen in progressive muscular atrophy. In the lower part of the cervical portion there was a small microscopical patch of sclerosis.

The Administration of Morphia Hypodermically in Large Doses.

In the *Practitioner*, June, 1880, Mr. Park reports several cases illustrative of the advantages of using morphia very freely. In Case 1, a strong, healthy woman, aged thirty-six, who was admitted with delirium tremens, he gradually injected three grains before producing any effect. After a good sound sleep she awoke well. Two days subsequently she became again noisy and restless; he therefore injected two grains without producing the slightest effect. For two days she continued unmanageable, when five grains of acetate of morphia were injected deeply, under the subcutaneous fat; half an hour afterwards she dropped asleep, and slept till morning. In another case, a man suffering from sciatica, three-and-a-half grains were necessary before relief could be obtained. Sleep was never produced as a direct result of the injection, so far as could be noted. Occasionally, large doses appeared to act as purgatives; certainly it cannot be averred that the constipation was increased either by large or small doses. In a case of granular Bright's disease, where large doses were frequently given, and, at times, continuously, for the relief of insomnia and orthopnea, not only was the palliation greater than expected, but the patient's life was greatly prolonged. Several other cases are quoted at some length, showing how beneficial large doses often proved. Mr. Park's conclusion is that the sedative, anodyne, and hypnotic effect of morphia do not go together. Hypodermically administered, and even in large and toxic doses, the hypnotic result may not be obtained. In certain cases of mania, the tolerance is very great. In practice it often happens that three minutes to half an hour elapse before the full effect of an injection becomes manifest, and often minutes elapse before the patient himself is conscious of any effect. It is remarkable that in no one of the cases in which Mr. Park used the very large doses recorded did any of the ordinary phenomena of narcosis ensue—a very different result to that which would necessarily follow administration of like doses by the mouth or rectum.

Physiological Test of Intoxicants.

Dr. Shorthouse says, so the *British Medical Journal* informs us, that if a man partake of too large a quantity of good sound wine, or malt liquor, he usually staggers about from side to side, his gait is very unsteady, and if he come to grief and to Mother Earth, he generally falls on one side or the other. If he take too much whisky, especially that abomination which goes by the name of Irish whisky, he is almost certain to be seized with an irresistible impulse to fall forward on his face. If he get drunk on cider or perry, the latter more especially, he is certain to fall down suddenly on his back, and apparently without any previous warning. He once saw a number of men who had made too merry at a harvest feast all fall down on their backs, get up again, and fall down again in the same manner. He had never witnessed anything of the like kind before; and was not a little amazed as well as amused. The farmer, who was a very shrewd Herefordshire man, told him that that was the effect invariably produced by perry, of which his men had that day partaken liberally. He has since that time seen several isolated cases, which have corroborated the farmer's version of the action of an overdose of perry or cider. Habitual drinkers of cider or perry are more liable than other persons to paralysis of the limbs; probably this may be due to the sugar of lead with which some cider-makers "perfect" their beverage. It would appear then, according to this very curious but very doubtful observation, that the various drinks act on different parts of the cerebro-spinal system which preside over locomotion, or act upon the various parts in a different manner, or why these varieties in the method of falling?

The Regulation of Body Heat.

A Fränkel, in *Centralblatt für die Med. Wissenschaften*, endeavors to solve the question, in what way and through what means, during changes in the production of heat in the body, the changes in the cutaneous circulation which make up the compensation through the giving off of heat, are effected. Heidenhain—as is well known—was the first to prove that the heat of the body is affected by the system of nerves under the sole agency of the circulation, by showing that, under reflex as well as indirect irritation of the medulla oblongata, the temperature of the blood in the large vessels decreases. The acceleration of the blood-stream, observed by Heidenhain, has been traced by Ostroumoff to an irritation of the nerve-fibres which dilate the vessels.

Over-production of heat in the organism under normal conditions, is dependent upon strong muscular activity and increased food absorption, both of which go hand in hand with increase of production of carbonic acid. The carbonic acid, however, directly irritates the centres located in the medulla oblongata, and consequently the formation of this end-product of the change of matter must be also connected with regulation of heat. As the accumulation of carbonic acid coincides with deficiency of oxygen, the author tried to decide his task by insufflation of differently combined quantities of carbonic acid, oxygen and nitrogen into the lungs of dogs. The insufflation was done with a compressible India-rubber ball, which was connected by a three-branched tube cannula, tied into the trachea of the animal, with gas-reservoirs, and with the atmospheric air. The cutaneous temperament of the paws and that of the rectum were measured. Some days before carrying out these experiments one sciatic nerve was cut through, in order to ascertain, by comparative measurements of both hind-paws, whether the changes in the cutaneous temperature, eventually to be observed under the influence of insufflation, originated from a changed innervation of the vessels remaining in connection with the centres or not. Three series of experiments showed, on the side on which the sciatic nerve was uncut, a rise of temperature; on the paralyzed side a standstill, or even fall of temperature. The inconstancy of the influence of the accumulation of carbonic acid on the irritation of the nerves dilating the vessels, observed by the author in other experiments, he traces back to prolonged restraint of the animals, and the simultaneous decrease of the temperature inside the body, and the continuous influence of the curare. The author is now of opinion that the increased production of carbonic acid in the organism, which occurs in increased temperatures under the influence of muscular activity, as well as of absorption—absorption of food—is one of the factors which regulates the activity of the nervous apparatus that governs the extent of the production of heat.

Hygienic Screens.

Mr. Wentworth Scott, says the *British Medical Journal*, recommends the employment of slag wool or silicate cotton, as a convenient disinfectant or odorizing medium. By impregnating the silicate with carbolic acid, thymol, or iodine for instance, and passing a current of air through the mass, which for such purpose is conveniently

contained in a box or case, the opposite sides of which are perforated, the air then will be incapable of communicating disease-germs, however foul it may previously have been. If eucalyptus oil, or other odorous substance be substituted for the preceding, the wool will impart an agreeable fragrance to the air-currents passing through it. The silicate may be renewed at any time, even if clogged with much dust and organic matter, by simply baking in a hot oven for a short time, and can then, of course, be charged again. Mr. Scott proposes to use these hygienic silicate screens in connection with the doors and windows of hospitals, schools, public buildings, and private dwellings. A form of safety respirator, for the use of nurses and others, on the same principle, is also suggested.

The Treatment of Diphtheria by Carbolized Camphor.

M. Peraté has, according to *Bulletin de Thérapeutique*, July 15, 1880, for the last two years used carbolized camphor for the treatment of diphtheria. He paints the surface with a pencil dipped in the following mixture: Carbolic acid, 9 grams; camphor, 25 grams; alcohol, 1 gram, diluted with equal parts of the oil of sweet almonds. The paintings are made every two hours in the day, and every three hours in the evening; then, after some days, they are divided by periods of three, four, or five hours, according to the improvement of the patient. These paintings are made over the whole extent of the false membranes, and with troublesome children the pencil is plunged as deeply as possible to the bottom of the throat, being of course, previously drained. The mixture has an extremely disagreeable taste, to which, however, the patient soon becomes accustomed. M. Peraté has been very successful with this plan of treatment.

The Treatment of Perspiration of the Feet.

Dr. Ortega, in *Le Praticien*, advocates the use of a solution of chloral in this affection. A patient of his, a strong man working in an ice manufactory, suffered from it in an extreme degree, so much so that his fellow workmen would not work by his side. The epidermis of the soles of the feet was white, as if macerated; there were small ulcerations in the furrows, and also around the nails. The odor was overpowering. Dr. Ortega prescribed baths of a solution of chloral, one in fifty, and wrapping the feet in a cloth dipped in a similar solution. Two days after the smell had disappeared. Six days

later, the treatment being continued, the ulcerations were less moist and covered with a layer of epidermis.

CORRESPONDENCE.

Professional Ethics.

ED. MED. AND SURG. REPORTER.

An editorial article in a former number of your valuable journal pictured in appropriate manner the position of the city physician in pursuit of professional business at the various places of summer resort. The means adopted by the wandering Esculapius to secure for himself the visitor who shall help him to pay his bills as he sojourns in the midst of pleasure and practice, and the position of the unfortunate country physician subjected to the lowering estimate of the *Eminent* from abroad, were no imaginary pictures. As corroborative of your statement, allow me to relate the following incident, which occurred a few days since, and "points a moral," as well as "adorns a tale." A gentleman at one of the hotels had been *unfortunate* enough to fall into the hands of a resident practitioner, and was being waited upon by one of the hall boys of the house.

"Who is your physician sah?" queries the boy.

"Dr.—one of the resident physicians," replied the gentleman.

"Is he one of de *lower order* of physicians in de village? I knows a good physician sah, stopping at the Grand Union, from New York sah."

"Yes?" replies the gentleman. "Who is he? "Dr.—," naming a New Yorker, in regular attendance upon our summer season, in pursuit of professional business.

The gentleman expressed his satisfaction with his present attendant, and the representative of the physician of a *higher order*, retired. "Verbum sat."

E. X. P.

Saratoga, July 31st, 1880.

The Quinine Mania.

ED. MED. AND SURG. REPORTER.

Forty years ago the sulphate of quinine was used as an antiperiodic, in diseases assuming the periodic form, and, occasionally, as a tonic in other diseases; now it is a general panacea; for where is the disease in which it is not used? Young and old, weak and strong, get their quinine. If the infant a few weeks old sneezes, the mother or nurse cries out, "the baby is getting cold and must have some quinine;" and quinine it gets, as regularly as its nourishment. The elegant lady, who, in consequence of her indolent habits or the exposures attendant on fashionable life, does not feel quite well, reclines on her lounge, or lolls in her easy chair and calls for her "keeneen."

After describing the diseases in which quinine is useful, Dr. Geo. B. Wood wrote, "but in all these cases it greatly behooves the physician to examine well the condition of the system, and

before resorting to the tonic (quinine), to ascertain the real existence of an enfeebled condition of the functions, and the absence of such local irritations or inflammations, especially of the stomach or bowels, as would be likely to be aggravated by its use."

Who stops to do this now? If a patient's temperature is too high, or too low, quinine brings it to the normal standard all the same. Irritable brains or stomachs must take care of themselves, for quinine is infallible.

The tyro, need not trouble himself about the therapeutics of quinine. If we accept the practice of the present day, it is omnipotent for good and at the same time harmless.

Who can say that the brain troubles of typhoid fever are not exaggerated by the excessive use of this remedy in that disease?

Is there no risk in giving it to the infant, whose brain is already predisposed to diseased action by teething, etc.?

This general use of quinine is a fine thing for the manufacturer, but for the physical and financial benefit of the community, let us stop the unnecessary use of this drug.

GEO. W. SNOWDEN, M.D.

Waterford Works, N. J.

NEWS AND MISCELLANY.

Famine in Asia Minor.

Recent news from the famine-stricken districts in Asia Minor, says the *Lancet*, August 21st, 1880, give a deplorable account of the state of the populations in the districts between Erzeroum and Bayazid, and in the districts of Van and of Mossul and the intermediate tract of country. The scarcity of food in these localities is extreme, and in many places the people have been trying to support life on wild herbs. The deficiency and high price of grain has, moreover, led to not more than a fourth of the usual amount being sown. The destruction among the flocks in the affected districts, partly from the severity of last winter and partly from the want of forage, has been enormous. It is estimated that half the oxen and three-fourths of the sheep have perished. Deaths from starvation have been very numerous, and the evidences of chronic starvation among the survivors are shockingly apparent. Typhoid, typhus, dysentery, scurvy, cerebro-spinal, meningitis, and small-pox are mentioned as the most prevalent diseases in these districts, and fears are entertained of some localities that, unless the Ottoman Government interfere with relief, worse evils from want than have yet been experienced are still in store.

Antivivisectionists.

The memorial recently presented to Mr. Gladstone, urging him to do all in his power for the absolute abolition of vivisection, was signed by "one hundred representative men," among them Cardinal Manning, Prince Lucien Bonaparte, Alfred Tennyson, Robert Browning, James Anthony Froude, John Ruskin, the head-masters of

Rugby, Harrow, and seven other large schools; twenty-one physicians and surgeons, and thirty-seven peers, bishops and members of Parliament. The memorialists take the ground that vivisection, even with anæsthetics, should by law no longer be allowed, and they quote the opinions of Sir William Fergusson, Sir Charles Bell, and Dr. Syme, that "it has been of no use at all and has led to error as often as truth." They add, that the utility, if proved, would not, in this case, excuse the immorality of the practice.

Dr. Leffingwell's paper, "Does Vivisection Pay?" which recently appeared in *Scribner's Monthly*, excited much discussion in London papers. It is said that Dr. Wood's reply, in the September *Scribner*, presents the other side with equal force.

Professor Gross.

The safe return of Professor Gross from his trip across the ocean will be welcome intelligence to all our readers. While in attendance upon the meeting of the British Medical Association the worthy Professor was the recipient of a high honor from the University of Cambridge. On August 11th, at 12.30 p.m., Prof. Gross, in company with several other distinguished members of the medical profession, had conferred upon him the degree of Doctor of Laws. It will be remembered that upon a former occasion he had received the title of Doctor of Civil Law, from Oxford. May he long live to enjoy his well merited honors.

OBITUARY NOTICE.

Philip De Young, M.D., one of the oldest practicing physicians in Philadelphia, died suddenly at his residence, 242 North Fifth street, recently. Dr. De Young was born in Montgomery county, in 1809, and studied medicine in this city, graduating from the University of Pennsylvania in 1838. He was immediately appointed physician of the Lehigh Navigation Company, through the influence of the late Asa Packer. Desirous of a wider field of action, Dr. De Young came to Philadelphia, in 1844, and has since been in active practice here. He performed a surgical operation on the day preceding his death.

QUERIES AND REPLIES.

Dr. Geo. P. Oliver writes:—

Dr. E. M. M., of New York State, will find the following an excellent remedy for "itching of the scalp and falling off of the hair:—"

R.	Hyd. chlorid. mlt.,	3j
	Glycerine,	f3j
	Cerat. plumbi subacetat.,	3j
	Ung. aquæ rosæ,	3j.
		M

Apply morning and night.

A Subscriber wishes to know if there is any way of entirely eradicating the Pin-worm (*ascaris vermicularis*), in a man fifty-seven years of age, who has been troubled with worms for seven years, and tried all the usual remedies without any benefit.

DEATH.

THOMASON.—At his late residence, Ferrineville, N. J., on Friday morning, Aug. 20th, Thomas J. Thomason, M.D., aged 47 years.